On the Determination of the Secular Acceleration of the Moon's Longitude from Modern Observations.

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In 'Proceedings,' Ser. A, vol. 95, pp. 300–302, Mr. Nevill applies to a set of observed errors of the Moon's longitude a change of the form $a+bt+ct^2$ and argues that the result shows a total absence of any difference between the theoretical and observed values of the secular acceleration in the modern observations extending from 1640 to 1915. The value of c in the above expression has been so chosen that the value actually used shall be that given by theory.

If the result of the application of such a change had been the reduction of the errors to small quantities which had apparently no systematic character, there might have been some foundation for the statement, but this is far from being the fact. It has long been well known, and it is again evident from the table of values which Mr. Nevill gives, that the errors show a periodic character and are so large that no conclusions can be drawn concerning the coefficient c until the nature and quantity of their periodicity has been determined. In order to avoid misconception it should be stated that a, b are constants which must be determined from observation and that large changes in their values are possible without sensibly affecting other portions of the theoretical expressions for the Moon's co-ordinates. Hence, it is usual in making any alteration in c to so determine a, b that the observations are best represented by the formula.

The main question involved is that of the representation of a curve of limited length by a formula, to a given degree of accuracy. A simple illustration is furnished by the expression $2''(t^2-1)$, where the unit of time is a century and the epoch is 1800. The changes produced at 1900 and 1700 by this formula are zero and the greatest change between 1915 and 1640 is at 1800 where the change is $-2''$. A glance at the numbers in Mr. Nevill's column $H\alpha$ is sufficient to show at once that the application of such a formula does not alter the periodic character of the errors, and it is known for theoretical reasons that it cannot do so, as long as knowledge concerning the periodicity of the errors is absent.

The fact is that the precise value to be attributed to the coefficient c is inseparably bound up with the representation of the remaining errors so long as the principal part of these errors can be represented by a periodic term

with a period of a length similar to that covered by the series of observations. The question has been fully dealt with by Tisserand in the third volume of his 'Mécanique Céleste.' In vol. 72, p. 708, of the 'Monthly Notices of the Royal Astronomical Society,' I have given a complete numerical illustration, the results of which can be very briefly stated. Let

$$A = 1{\cdot}88''\,(t-0{\cdot}27)^2+12{\cdot}95''\sin(131^\circ\,t+100{\cdot}6^\circ),$$

$$B = 1{\cdot}56''-1{\cdot}43''\,(t-0{\cdot}27)+11{\cdot}15''\sin(138^\circ\,t+99{\cdot}4^\circ),$$

where t is the time reckoned in centuries from 1800. It was shown that the difference (A−B) will never exceed 0·1″ between 1710 and 1930 or 0·5″ between 1620 and 1950. Even so considerable an alteration as $5{\cdot}4''t^2$ was, by similar methods and by a proper choice of the periodic term, and of the coefficients of t^0, t', shown to produce changes less than 0·3″ between 1710 and 1930 and changes less than 1·6″ between 1620 and 1950. Hence the coefficient of t^2 cannot be obtained from the modern observations until the source of the periodic term is known and its constants determined with very considerable accuracy from the theory. Even when either A or B has been applied to the longitude, the remaining errors require for their representation at least three periodic terms with coefficients of some seconds, so that the difference between adopting A or B cannot be settled on the basis of the observations.

At the end of his paper Mr. Nevill states that the large outstanding fluctuations are due to Hansen's erroneous values for the terms of very long period, and that they disappear so soon as these are replaced by their correct values. He apparently ignores the fact that the work of Radau, Newcomb, and myself shows substantial agreement, both theoretical and numerical, in the conclusion that these outstanding errors cannot be explained in any such way. In order to justify such a statement in the absence of any published and detailed investigation of his own, it is necessary that he should show in what manner we have failed to perform our work correctly.